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DISCUSSION THE METHOD OF EXAMINATION

By E. B. TITCHENER

During the past few years we have published, from the Cornell Laboratory, a number of studies in which we sought to test the 'method of examination' or 'method of questions.' We owe the suggestion of this work, for the most part, to the publications of the Würzburg school. It is clear that the method, if it is reliable, greatly extends the scope of experimental psychology; but it is also clear that, if the results obtained by Külpe and his co-workers are accepted, the system of psychology must be rebuilt from the ground up. We were ourselves unable, when we repeated their experiments, to confirm some of the most important theses of the Würzburg investigators; we thought that we found in their method a definite and familiar source of error; and we therefore saw no reason for an immediate rewriting of our psychology. It seemed best to suspend judgment until we had made trial of the method in our own behalf.

No laboratory, of course, can devote itself *in perpetuum* to a single method and a single range of topics. We have, I hope, given the method of examination a fair test; and we may now, I believe, summarise—in a tentative and provisional way—the conclusions that we have reached. That is the object of the present paper. And since a scientific standpoint is most sharply defined by contrast, I shall take account, in what follows, of the—uniformly unfavorable—criticisms which Dr. Koffka, of Giessen, has published in the *Zeitschrift für Psychologie* upon the studies to which I have referred. In matters of detail, some of Dr. Koffka's objections seem to me to be well taken, others appear positively to miss the point; I shall, however, try to subordinate minor differences to the main questions at issue. I have, on my side, this principal charge to bring against our critic: that he does not recognise the serial nature of our work; he does not see that we have been feeling our way to clearness, step by step and problem by problem; he does not realise that the progress from Okabe's article on *Belief* to my own discussion of *Description and Statement of Meaning* is as real—*si parva licet componere magnis*—as the progress from Marbe to Bühler. Otherwise he would surely have noted that the shortcoming of an earlier paper is redeemed, at least so far as intention and effort are concerned, by the plan of some later study.

Another introductory remark must be made. It is natural that a student, who has spent two or three years upon this method of examination—reading, criticising, observing, planning, interpreting—should, when he comes to publish, lay stress upon the positive outcome of his work. He has been no less patient, no less ingenious, no less single-minded than his colleague, whose study of sensation or perception has been based upon methods of a stricter type; and he is eager to show that his labor has not been wasted; he wishes to make his contribution to the store of psychological fact. It is,

nevertheless, not quite fair to take him at his word, and to judge his results as they are offered. The reader with a wider perspective will understand that the general method is itself on trial, and that, while the statements of the author's *Summary* are a proper subject of criticism, yet the critic fails of insight if he forgets their genesis, their attainment by a practically unaided observation. We shall never discover, by the naked eye, the details that lie plain in the field of the microscope; but where the microscope cannot be employed, a gross description may have real scientific value. We shall go astray if we bring the eye into comparison or rivalry with the microscope; but the critic is also at fault when he confuses the two orders of observation, when he judges them both by the same standard; for results, after all, are a function of method.

*Okabe on Belief.*¹—In his study of belief, Okabe had six observers, four trained and two relatively untrained.² His method was to lay before these observers, in visual or auditory form, sentences or mathematical expressions which were calculated to arouse belief or disbelief; or, later in the work, to present sentences or mathematical expressions in pairs, the one member of which should arouse belief, the other disbelief.³ The observers were to attend and to understand; if then belief or disbelief appeared, they were to close their eyes and to dictate a report of their experience. No time-limits were set, and no time-records were taken.⁴

The main results of the study were three. The first was that the experience of belief may be either explicit or implicit; that is to say, belief may appear as a complex course of specific content-processes, or may be bound up with, incorporated in, a particular consciousness, with no conscious representation beyond the mode of occurrence of this consciousness itself. The distinction thus drawn is no new psychological discovery; the reports of Okabe's observers confirm and extend observations of Ach, Messer and Störring;⁵ but the independent confirmation, and the extension to the untrdden field of belief, are surely worth while. The second result was that belief and disbelief are consciousnesses of the same kind. This, again, is not a new discovery. "The true opposites of belief, psy-

¹ This JOURNAL, xxi., 1910, 563-596.

² Koffka remarks that the reports of the untrained observers, which "differed in important points from those of the others, were dismissed as *Kundgabe*" (*Zeits.*, lxiii., 1912, 398). The facts in the case will be found in Okabe's paper, 575 f.

³ Okabe speaks here of "a method [not *the* method] of paired comparisons" (580). I do not know why he should not; but Koffka objects to the name (*Zeits.*, 397). Okabe gives (*loc. cit.*) the reasons for his change of method.

⁴ Koffka thinks that the duration of the single test, and the number of tests made at a sitting, should both have been recorded (*Zeits.*, 397). The criticism is just. We supposed, at the time, that we might obtain more complete descriptions if the observer knew that he was *not* to be timed (*cf.* Wundt's criticism of 1907, *Kleine Schriften*, ii., 1911, 277, 281, 293); but later experience showed that observers very soon grow accustomed to the stop-watch. The sittings were always of an hour's length, and our papers show the number of tests made in every hour; we did not find that this number was significant; but the fact should have been stated.

⁵ *Op. cit.*, 592 f.

chologically considered," says James, "are doubt and inquiry, not disbelief;" and Bain had said the same thing before him.⁶ Okabe thus confirms, by the evidence of his observers, an opinion already expressed as a personal conviction by psychologists of note. The third result—which is, in fact, a set of results—consisted in a rough analysis and differentiation of the experience of belief as it varies with type, affective disposition, and so forth. Here is new material; and its value is the value that attaches to a first, direct, observational report of a fairly complex situation.

Koffka, however, has two sweeping objections. In the first place, Okabe's trained observers show theoretical bias, and his own conclusions rest upon an arbitrary selection of reports.⁷ I can hardly be expected to meet this criticism otherwise than by referring the reader to the article itself. In the second place, we have not attempted a real analysis: we do not say whether all our observers attached the same meaning to 'belief' and 'disbelief'; we do not say what visual quality (*Sehqualität*) is involved when an observer 'sees' agreement or disagreement; and we do not undertake a special study of the *Aufgabe*.⁸

As regards the *Aufgabe*—I take up Koffka's points in reverse order—I grant that we leaned somewhat heavily on the work of our predecessors in Germany; we sought, later, to correct this mistake. As regards the 'seeing' of agreement, I can only suppose that our critic has, in some extraordinary fashion, misread Okabe's statements. In his first analytical summary of the reports obtained from one of his observers, Okabe speaks of "a 'perception' of agreement or disagreement, of harmony or disharmony, which was difficult to analyse, but seemed in every case to be preponderantly visual." So far is he from reference to a 'visual quality' of agreement that he hesitates to use even so loose a term as 'perception.' Later on, when the reports grow fuller, he allows himself to speak outright of the perception of agreement; and finally, when he summarises the whole body of results for purposes of confrontation, he says: "the core of belief-disbelief is to be sought in the arrangement and behavior of visual images."⁹ Is it necessary to point out that arrangement and behavior are spatial and temporal affairs, independent of sensible quality? or that there is a progressive analysis as this observer gains in practice?

There remains the objection: "nirgends ist die Frage aufgeworfen, ob alle Vpn. dasselbe unter belief und disbelief verstanden." The critic is mistaken: there is a passage in which that question is explicitly raised.¹⁰ I do not rest, however, in this formal reply. We find, in the reports, cases of a passive acceptance which follows upon understanding; we find typical consciousnesses which we and the observers call belief and disbelief; and we find consciousnesses of certainty, of positive conviction. We find that there are various shades or *nuances* of belief, which the observers seek to express by adjectives. All these things are set down, in such detail as the method permits. And our object was, of course, to learn what sorts of consciousness are covered by the term belief, as this term

⁶ *Ibid.*, 564 f.

⁷ *Zeits.*, 397 f.

⁸ *Ibid.*, 398 f.

⁹ *Op. cit.*, 570, 577, 583, 589, 592.

¹⁰ *Ibid.*, 576.

is ordinarily employed,—not to erect a norm of belief, to which the observers should be brought to conform.

*Clarke on Conscious Attitudes.*¹¹—Clarke's aim is to bring the conscious attitudes—"certain large and comprehensive experiences, not evidently imaginal in character"—to the test of introspective observation, and thus to discover whether or not they are analysable. Her early tests yield a number of incidental analyses, of which she writes: "Many of these incidental analyses are, evidently, imperfect. Even at the best, the observers might report in large and sweeping phrases, such as 'bodily attitude' or 'organic sensation.' There is, however, no doubt that the reports were intended at the time to represent the attitudes themselves, and not merely incidental or concomitant occurrences." And she supports this position by the direct acknowledgments of her observers.

"It is obvious," says Koffka, "that analysis meant, for the author and her observers, nothing else than the exhibition of the sensory contents present at any given moment. . . . These sensory contents may be irrelevant to the thought, or may be the necessary condition of the arousal of a thought, or may finally be the thought itself. Why this third possibility should alone be recognised, neither the author nor her observers explain."¹² The reply is that this possibility was not alone recognised. It was precisely because there were other possibilities that the observers were instructed, at the outset, to give complete introspections, and were confronted, later on, "with an outline of their reports upon various attitudes, and were asked to say whether, so far as they could remember, the analyses were, as analyses, correct. The regular answer was that they were correct, and in several cases the observer added, of his own accord, that he could reproduce the attitude, at the moment, and that it corresponded with the analysis given." The sensory contents were, then, certainly not irrelevant; nor were they, so far as the observers could tell, the condition of some further and specific conscious contents; they were the given conscious factors of the attitudes themselves.

But, the critic goes on, "it is sheer enigma that an observer can say, by direct observation, that 'approval' is 'pleasantness with some general kinaesthesia.' Pleasantness and general kinaesthesia are first of all just that,—pleasantness and general kinaesthesia."¹³ To be sure they are, when you treat them in separate chapters of a text-book. But the objection, taken literally, denies the possibility of psychological analysis in any field. What we have in the present case is simply this: that under the instructions given, and under the conditions of the experiment, the 'attitude of approval' factorises into pleasantness and some general kinaesthesia. Nobody asserts that the analysis is adequate; Clarke herself, as we have seen, regards her incidental analyses as incomplete; the point is that the attitude, in the particular circumstances, does not wholly resist analysis, but factorises in the manner stated. Clarke then proceeds to more detailed analyses, which the reviewer dismisses as worthless.

But, once more, Clarke speaks of imagery as "carrying thought. Yet in that case thought would be, after all, a specific conscious

¹¹ This JOURNAL, xxii., 1911, 214-249.

¹² Zeits., lxiii., 1912, 219.

¹³ *Ibid.*, 219.

contents; and this, again, is directly denied."¹⁴ Can the criticism be seriously intended? It would mean that every psychological term must find representation as a *besonderer Bewusstseinsinhalt*. It would mean that when, for instance, we have analysed an 'emotion,' we shall have to add to our analytical results a specific conscious contents 'emotion.' And more than that: it would mean that every general term in logic or ethics or aesthetics must have its special conscious representative: whatever psychological analysis may discover in the judging, approving, appreciating consciousness, there must always be a specific contents of judgment, of approbation, of appreciation! For it is clear from the context that the thought which Clarke's imagery 'carries' is understood by her to be logical thought.

I can only suppose that the reading of Clarke's paper made upon the reviewer a generally unfavorable impression, and that, when he came to write his review, he was more concerned to express dissatisfaction than to work out psychological principles. At all events, I cannot see that a system of psychology would be possible, if these principles are to stand. I hear the notes *c-e-g* struck on the piano. I may recognise the chord, as the common chord in the key of *c-major*. I may also analyse it into tones and noise; I find, perhaps, a form of combination, an elementary aesthetic feeling, an organic reverberation. Is it "sheer enigma" that a practised observer can resolve 'the common chord in the key of *c-major*' into these components? and will Koffka deny the possibility of psychological analysis at large? Or again: I say that my discrimination, in certain terms of a psychophysical series, was based upon absolute impression. Would discrimination, in such a case, "be, after all, a specific conscious contents," although "this is directly denied?" Koffka must, apparently, reply in the affirmative. I submit, once more, that the psychological principles which underly his criticism are at least unfamiliar, and that a mere appeal to them as axiomatic does not justify his adverse judgment of Clarke's work.

The remainder of Clarke's article, which is dismissed as beneath consideration, is taken up with a demonstration of graduated steps from clear imagery to 'imageless thought,' and with detailed analyses, explicit and genetic, of particular conscious attitudes. Clarke, I may add, discusses the *Aufgabe*; registers the times of reaction; and often mentions the place of a report in a sitting or series.¹⁵

*Titchener on the Psychological Self.*¹⁶—I should not have been surprised if our critic had ignored my note on the *Consciousness of Self*. As it is, however, I must defend myself against a hailstorm of critical condemnation. I had found, in a current text-book, the statement that 'I am always, inattentively or attentively, conscious of myself, whatever the other objects of my consciousness.' This statement was at variance with my own experience, and I wished to put it to a preliminary test. Koffka objects, first, that the concept of self, "which springs from a whole number of sources, but assuredly not

¹⁴ *Loc. cit.*

¹⁵ Okabe published in October 1910, Clarke in April 1911. Koffka's reviews were apparently sent to the editor at the same time: that of Clarke came out in November, that of Okabe in December 1912. May I not justly express surprise at Koffka's failure to point out that some of the glaring defects of Okabe's method had been corrected by Clarke?

¹⁶ This JOURNAL, xxii., 1911, 540-552.

from those of a scientific psychology, is employed simply as a psychological concept."¹⁷ That is true. It was the psychological self that I was concerned with, the self that is discussed in the psychologies as part of the subject-matter of psychology; all other selves were indifferent to me. If I have erred, a great many others are also at fault. Should not the blame be shared? and should we not be told why a psychologist may not investigate the conscious representation of a concept—whatever its origin? Koffka, to be sure, adds that "wir die wichtigsten Aufschlüsse wohl von der Pathologie zu erwarten haben." Let that be granted: and I, at any rate, have nowhere denied it: still, is it, in the present connection, anything but a paralogism? For the generalisation that I was combating is certainly not derived from pathological sources.

"It is simply assumed," the critic proceeds, "that everybody understands by the word self-consciousness not only a perfectly definite experience, but also an experience that is somehow the same (*irgendwie gleichartig*)."¹⁸ The objection, if it can be urged with any show of reason at all, affects the statement that I was examining rather than my own procedure. For my second question called for a description of the self-consciousness, which should be made "as definite as possible. Is the consciousness of self explicit (e.g., visual image, organic sensations)," I asked, "or implicit (intrinsic to the nature of consciousness, inherent in the course of consciousness)? Can you bring out the character of the self-consciousness by comparing or contrasting it with other phases of a total consciousness?" Neither in the formulation of this question nor in my treatment of the reports did I make the assumption attributed to me.¹⁹

"As we have to do in the present instance with an experience of extremely complicated structure," the reviewer continues, "this [uniformity] is not self-evident, but on the contrary is highly improbable." I can, again, only wonder at my critic's psychological principles. Genetically, the concept of the self may be of an extreme complexity; but is there any reason *a priori* why, in the consciousness of the educated adult to-day, it should show an "extremely complicated structure?"

There remain two objections of a more practical kind. "There are," observes our critic, "far simpler experiences in face of which the author's method meets with flat failure." I myself refer to cases in which a somewhat similar method proved unreliable, and mention the additional safeguards upon which I rely.²⁰ Koffka, however, is speaking of sources of error other than those that I had discovered. He must, therefore, have in mind cases in which my method was exactly—and fruitlessly—anticipated. I do not know of them; and he, unfortunately, does not give references. He objects, finally, that the method "introduces into the laboratory the old psychology of reflection."²¹ I should say that the method furnishes a rough way of discovering whether the old psychology of reflection is already

¹⁷ *Zeits.*, lxiii., 1912, 212.

¹⁸ *Op. cit.*, 545 f., 550 f.

¹⁹ *Ibid.*, 540.

²⁰ It is characteristic of the criticism evoked by this method of questions that the charge which Koffka here brings against me, in particular, is precisely the charge brought by Wundt against the Würzburg school in general: see *Physiol. Psychol.*, iii., 1911, 551 ff., esp. 553 f.

entrenched in the laboratory; I do not see how a request for direct observation, with a judgment of presence or absence, could of itself introduce that psychology. I return to this point later.²¹

*Jacobson on Meaning and Understanding.*²²—The novel feature of Jacobson's work on the perception of letters and the understanding of words and sentences is the attempted separation of 'description' from 'statement of meaning.' The observers were required 'to put direct description of conscious processes outside of parentheses, and statements concerning meanings, objects, stimuli and physiological occurrences inside.'²³ Jacobson, now, is taken to task, first of all, because he further instructed his observers to report their experiences in strict temporal order. 'It has not occurred to the author that he could do better by setting various temporal limits, objectively, to the process under examination.'²⁴ This is a little harsh, when even Okabe had had recourse to fractionation!²⁵ But, indeed, the critic has not read with sufficient care. Fractionation may give you the objective temporal order of occurrence; but it tells you nothing of the observer's temporal attitude during an extended report. And Jacobson was interested in the method; he wished to find out whether the time-relations noted in the extended reports of previous investigators were reliable.²⁶ A second point against him—that the nature of his 'statements of meaning' is not clear²⁷—must be acknowledged as fair criticism. It should be said, however, if we are to continue fairly, that Jacobson was aware of this defect, and himself calls the reader's attention to it; and also that he expressly raises the question of the difference between associates which carry meaning and associates which do not.²⁸ Koffka charges, thirdly, that the method of reinstatement or repetition betrays ignorance of "the rules for the use of introspection."²⁹ As if these *Vorschriften* could be written down with mathematical accuracy, and as if a rule did not vary with variation of the conditions of observation! Jacobson refers to Wundt's article of 1907, and was familiar with the discussion of 1888.³⁰ He wished to learn whether, if the original conditions of the observation are restored,³¹ the method of reinstatement is still

²¹ P. 440 below.

²² This JOURNAL, xxii., 1911, 553-577.

²³ *Ibid.*, 555.

²⁴ *Zeits.*, lxiii., 1912, 380.

²⁵ *Op. cit.*, 567.

²⁶ *Op. cit.*, 553 f. Jacobson found also that the temporal instruction "was of material aid in the correlation of 'process' and 'meaning.'

²⁷ *Zeits.*, 381.

²⁸ *Op. cit.*, 556 f., 566 ff. A more sympathetic critic, who had observed the serial nature of our work (p. 429 above), would perhaps have anticipated a further study, dealing with this outstanding question.

²⁹ *Loc. cit.*

³⁰ *Op. cit.*, 556; W. Wundt, *Selbstbeobachtung und innere Wahrnehmung*, *Phil. Stud.*, iv., 1888, esp. 298 f.

³¹ *Ibid.*, 555. Koffka's further criticisms—as that Jacobson should have consulted the reports of tachistoscopic experiments—seem still to show that he misses the writer's interest in the general method. I am inclined to agree that Jacobson's modifications of the method of questions, ingenious as they are, do not materially advance it; but I think that they were worth trying; and at any rate there could be no question of a recourse to the tachistoscope. Again, however,

worthless. He found that his observers did not hesitate to report failures; that sometimes specific differences were noted between the two experiences; and he decides on the whole that "the results are encouraging, though we offer them only as a first contribution to the settlement of the question." These careful statements do not, surely, show "ignorance of the rules for the use of introspection!"

*Titchener on Description and Statement of Meaning.*³²—In this paper I recur to some of the points which Jacobson had left obscure, and in particular to the question of the nature of his bracketed 'meanings.' I seek to characterise the attitudes implied in, or demanded by, the two modes of report which he had called for; and I find that the one is the attitude of descriptive psychology, the other that of logic or of a logical common sense. The distinction, then, is now no more difficult for us than for Bühler,³³—though, as I need not say, it is not Bühler's distinction. And with it we seem to have gained the right to draw up that provisional and tentative summary of which I spoke at the beginning of this article,—the summary of our conclusions in regard to the method of examination. There are, it is true, many questions which still remain without answer;³⁴ and our own use of the method has, as the preceding paragraphs show, been roundly condemned. But the condemnation rests, in good part, upon psychological principles which are anything but secure; and I hope, in despite of it, that the following remarks may not be without value.

We are not to suppose that the method of questions has reached the limit of its usefulness. We may expect, on the contrary, that future work will extend both the range of its application and the variety of its forms. There seems, however, to be no likelihood that we shall gain from these extensions any power or any knowledge that we have not, in principle, already obtained. The method, as method, has been sufficiently tried, and may now be appraised.

I. I begin with a negative. It does not appear that the method of questions will ever avail, of itself, to settle disputed questions of a systematic kind: that it will enable us to decide, *e.g.*, for or against the distinction of 'act' and 'contents,' or to compose the issue between imageless and imaginal thought, or to prove or disprove the existence of a 'form of combination.' For, in the first place, the conduct of the method, the empirical procedure, is always open to a criticism which derives its canons from the more rigorously experimental ways of working that have established themselves in the fields of sensation and perception. Criticism of this sort will hardly influence the author of any given study; it will seem to him to be irrelevant, off the point; when he adopted the method, he will say, he took it with full acceptance of its limitations; he turned his back deliberately upon the refinements of experimentation; to pick minute flaws, to pepper the work with objections of detail, is only to do what he could do for himself; it shows a misunderstanding of his aim and intention. But then this same author, when it is his turn to criticise, finds himself in a quandary. Unless he is

it is characteristic that Koffka's recommendation is identical with that of Wundt to the Würzburg group: *Kleine Studien*, ii., 1911, 291.

³² This JOURNAL, xxiii., 1912, 165-182.

³³ *Zeits.*, lxiii., 381.

³⁴ See this JOURNAL, xxiii., 1912, 507; xxiv., 1913, 154.

to confine himself to a discussion of principles, he too must come down to detail; and there his opponent will meet him with his own retort. And so the critical volleys are fired, back and forth, and neither side will acknowledge a hit. The spectator, no doubt, will form a positive opinion; yet it may be questioned whether this opinion is induced directly and solely from the results of criticism. We have, in a word, a gross and loose method which is attacked or defended with all the art and skill that an experimental methodology can provide; and we cannot hope, under these conditions, to settle the kind of question that was raised at the beginning of this paragraph. But, in the second place, the results of the method—since, as I have said, results are a function of method—must themselves be gross and loose, capable of various interpretation. So an introspective report that is published in support of a certain thesis may be seized upon by a psychologist of different persuasion, and interpreted in a radically different sense; or the charge of 'arbitrary selection' may be laid against the presentation of results, as the charge of 'bias' or 'prepossession' may be laid against the conduct of the method. For these defects I see no promise of remedy in the method itself; and in so far my experience has brought me into agreement with Wundt.

II. I believe, nevertheless, that the method is of value. I can better show wherein this value consists, however, after I have characterised the method which I think will replace the method of questions as an accredited procedure in experimental psychology. Wundt, as is well known, would base a psychology of the 'higher' processes upon a study of mental 'products'—so that the psychology of thought would emerge, e.g., from a psychological study of language:³⁵ a direct experimental modification of these processes is for him impossible, and their direct observation is of necessity inexact. But while one will hardly find an experimentalist who denies the importance of *Völkerpsychologie*, one will hardly find one, either, who accepts Wundt's division of the science. And the method which we must look to, if we are to transcend this division, is—it seems to me—a method of the type of Ach's 'systematic experimental introspection,' a method, that is, which secures description under experimental conditions so strictly controllable that we may hope, by manifold repetition, to attain accuracy of report.³⁶ We cannot, in other words, dispense, in the study of the higher processes, with the experimental aids which have helped us to an understanding of the lower.

Ach's method, however,—in the particular form that he gave it,—has been severely criticised by G. E. Müller.³⁷ It is dangerous. Müller

³⁵ See, e.g., *Physiol. Psychol.*, iii., 1911, 554.

³⁶ N. Ach, *Ueber die Willenstätigkeit und das Denken*, 1905; *Ueber den Willensakt und das Temperament*, 1910. I say purposely "a method of the type of Ach's." The attempt to work out, in detail, the grounds of this qualification and the nature of the method contemplated would lead me too far afield, and would perhaps in any case be premature. It is plain, however that very many experimental procedures, over and above those employed by Ach, may be incorporated into the method. I suggest also that, in the sphere of the higher processes, direct may be supplemented by indirect modes of approach.

³⁷ *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, i., 1911, 137-143. I have discussed this criticism, from another point of view, in this JOURNAL, xxiii., 1912, 503 f.

remarks, for several reasons, to have free recourse to question and answer, aside from the fact that questioning unduly prolongs the report and thus puts too great a strain upon the memory of the observer. It is out of the question that the introspective report of a fairly complicated consciousness should be even approximately complete. The perseverative tendencies, on which Ach relies, are selective and at times misleading; and we have no proof that they are strengthened by the intent to observe. Finally, it is dangerous to suggest to the observer that the contents of the after-period are, as perseverative, identical with those of the experimental consciousness; we know of cases to the contrary; and the observer should therefore be instructed to report only such experiences as he remembers, with assurance, to have occurred during the experiment.

It is, now, a little disquieting to find that Müller's criticisms bear upon the 'systematic introspection' and not upon the 'experimental' of Ach's method; we seem to be back again in the fruitless discussions of the method of questions. But let us see! There is no physical compulsion to ask questions; the experimenter may rely upon the repetition, under identical or under modified conditions, of the task set to the observer. Or if, from lack of time or from the appearance of carelessness or of stereotyped reaction in the observer, questions seem to be indicated, then the reports that are obtained (and later reports that are possibly influenced) by questioning may be treated by themselves, and may be compared with the spontaneous reports of the same and of other observers.³⁸ Again, the requirement of a complete report may be so phrased that the observer understands it, simply, as the demand for a non-selective report; he is to give an account of everything that he can remember, and not to pick out the items that he himself thinks important. The idea that the contents of the after-period are necessarily the same as those of the experimental consciousness may be corrected, as Müller points out, by suitable instruction. On these three points, then, it seems possible to offer a straightforward reply to the critic, and to safeguard the method. The essential thing is, after all, that the method permits and prescribes a truly experimental control of conditions. The advantage is twofold. First, the repeating of the observation, and the varying of its conditions, allow us to test report by report; the method becomes, in a very real way, self-critical;³⁹ and, secondly, the standardising of conditions fulfills one of the chief requirements of a good method, that the observations may be exactly repeated by other investigators.

But I have said nothing of the perseverative tendencies, the foundation upon which Ach builds. Here we have, in fact, a disputed issue of systematic psychology; and here Ach's method, at any rate

³⁸ Cf. my remarks, *op. cit.*, 505 f., and the references given 503 f. It is noteworthy that E. Westphal, who employed Ach's method (*Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen. Arch. f. d. ges. Psych.*, xxi., 1911, 432), was careful to phrase his instructions in such a way that "the observers were held down to a report solely of what had, with certainty, been actually experienced" (434).

³⁹ Westphal's use of the 'synthetic method' (*op. cit.*, 359 ff., 397 f.) is a case in point. After reading certain things *out of* the reports (analytical method), Westphal proceeds to read these same things *into* the instructions of a new experimental series (synthetic method). The reports thus become, in so far, their own test.

in its present form, is plainly inadequate. It may be said, however, that the acceptance of perseveration, as a principle of explanation, is not essential to the use of the method, and that the study of perseveration, as a general term for certain observed phenomena, can be carried on in experiments of less complexity. While, then, it is regrettable that Ach, in the statement of his method, has laid stress upon perseveration,—since by so doing he has exposed himself to criticism which the method does not enable him to meet,—still the method itself does not stand or fall with his personal interpretation, and the rightness or wrongness of this personal view can be determined 'out of court' by other methods. Müller's criticisms do not affect the core of the method, which is, once more, the securing of description under rigorously experimental conditions.

III. I think, then, that the method which is to supplant the method of questions will be of the type of Ach's 'systematic experimental introspection.' And now I return to the method of questions itself.

In his reply to the objections urged by Wundt against the method of examination, Bühler remarks that his first paper was meant to furnish "a preliminary orientation in regard to certain fundamental questions of the psychology of thought."⁴⁰ This statement, literally taken, points out the chief use of the method: it enables us to make a first survey, to get our general bearings, in new fields of work. For many years, *e.g.*, experimental psychology looked a little nervously upon such things as doubt, wish, belief, desire, the formations that we now know as 'conscious attitudes,' and the method of examination has brought us face to face with these experiences, and has shown us where the problems lie, even if it has failed to furnish us with a satisfactory analysis. Ach is therefore right in ascribing an heuristic value to the method.⁴¹ It will always be of service where new ground has to be broken, and where the formations are so complex that an immediate recourse to experiment, in the strict sense, is forbidden.

That is, undoubtedly, the chief value of the method; there are, however, other ways in which it may be useful. Thus, we have seen that it is eminently provocative; it is potent to call forth criticism and counter-criticism; if it does not settle questions, at least it cannot avoid raising them. Hence, as the method itself shows where problems lie, so does the criticism which it evokes help toward the formulation of the problems, and show what pitfalls are to be avoided, what precautions taken, when the problems are approached in a properly experimental way. Criticism, I said above, has in general been sharpened to a point too fine for the method and results against which it was directed; within the boundaries of the method, therefore, it has been ineffective and unconvincing; but this same keenness and delicacy will prove of value when the investigator is passing from the method of examination to the other and more refined method of systematic experimental introspection. It is true,

⁴⁰ K. Bühler, Antwort auf die von W. Wundt erhobenen Einwände gegen die Methode der Selbstbeobachtung an experimentell erzeugten Erlebnissen, *Arch. f. d. ges. Psych.*, xii., 1908, 103.

⁴¹ N. Ach, *Willensakt und Temperament*, 1910, 16 f. It should be noted that Wundt admits the usefulness of the 'experiment without instruments' in cases where the problems are very simple or where but little work has already been done upon them: *Kleine Schriften*, ii., 1911, 274.

no doubt, that as the coarser method is reduced more and more to a means of preliminary exploration, its provocativeness will steadily decrease. But even if it produce less controversial writing in the magazines, it will continue to work as ferment in the group of co-workers in the laboratory.

I am thus led to a final point. All those who have worked with me by the method of questions have assured me that they have learned from it a great deal of psychology; some have even proposed that it be introduced into the undergraduate laboratory courses. To be set over against a complex experience, and to be asked for a complete description, is—these observers say—variously informing: now, perhaps, one realises the inadequacy of current terms and formulae; now, one learns how much of one's supposed psychology is a matter of borrowed categories or of logical reflection; now, again, one makes novel discoveries as to one's mental constitution; always one is directly impressed by the complexity and elusiveness of consciousness. This advantage must not, I think, be exaggerated. A stricter method might yield the same results; it is not necessary that, because conditions are more rigorously controlled, the experimenter do all the psychologising. And there is, besides, the danger, in this method of questions, that the lines of system harden, and that interpretation be too positive. So far as it goes, however, the judgment of my co-workers must be recorded in favor of the method.

A method which possesses an exploratory value, a critical value, and an educational value,—even if the two latter values are of a lower order than the first,—is not likely to disappear; the prospect, as I remarked at the beginning of this discussion, is that it will extend its range and multiply its forms. But a just appraisal will hardly give it rank with the approved methods of the science. The psychology of the 'higher' processes lies in the hands of *Völkerpsychologie* and of the method of 'systematic experimental introspection.'